

being the case with most of the Riviera torrents. For instance, the channels of the streams near Menton, Vintimiglia, and elsewhere, are far out of all proportion to the work they have to do. Take the case of the principal stream at Menton. At a distance of less than two miles from the sea where its bed is formed of rock, it has only a breadth of a few yards, and has no high flood-marks indicating that there is ever a great depth of water. If the stream is followed downwards from this point for less than a mile, the bed is found to open out to a breadth of from sixty to seventy yards. Between these points there are no tributary streams adding their waters to account for this increase. These large river-beds are caused by the nature of the country which these rivers drain. The country is very mountainous, the hill slopes are rocky and steep, large areas have no covering of soil, and what soil there is does not retain the water well. The result of this is, that when rain falls the water rapidly finds its way to the streams, and the same amount of rainfall is discharged by these streams in a few hours as is discharged in weeks by an English river draining the same area. This accounts for these torrents rising so "high" and falling so "low." It also accounts for them "rising" and "falling" rapidly.

But further, the great and unnecessary breadth of these torrent-beds where they approach the sea seems to be produced somewhat in the following way:—The valleys through which these streams flow descend rapidly from the mountains, but as they approach the sea their slope becomes much slower; the result of this is, that the gravel brought down by the river from its higher and more rapid reaches, is here deposited, on account of the water losing its velocity, and the bottom of the valley becomes filled with a bed of gravel, through which the stream winds sometimes in one part, sometimes in another. A very small cause being sufficient to make the stream "cut" into the gravel and alter the position of its bed, and cause it to flow in different parts of the channel at different times, but it almost never covers at one time the whole breadth of it.

That the bed of the principal stream at Menton is unnecessarily large, is evident from the fact that now, on account of the increased value of land, they are building a retaining-wall near the centre of the stream, and filling up about one-half of the river-bed for the purpose of cultivation.

Rivers similar to those of the Riviera are common to all mountainous countries, Britain not excepted. There is at least one salmon river in Scotland, which during the dry season may be walked across without wetting the soles of one's boots, all the water finding a passage among the gravel. Yet in Autumn, when it has fallen to "fishing condition," it is a stream of about thirty yards broad, and an average depth of about two feet on the fords. This river is also subject to great floods, which "come down" rapidly, and "fall" rapidly. It also has gravel deposits similar to those of the Riviera torrents, but in this case they are covered with soil and cultivated, and it is with the greatest difficulty and at great expense that the river is prevented from widening its channel to the proportions of those of the Riviera torrents.

JOHN AITKEN

Bellagio, Lago di Como, Italy

Method of Distributing Astronomical Predictions

I BEG leave to observe that the very useful method of distributing astronomical predictions over a given geographical area alluded to in NATURE, vol. xiii., page 71, and ascribed there to Mr. W. S. B. Woolhouse, was already proposed by my father, J. J. von Littrow, in his treatise, "Darstellung der Sonnenfinsternisse vom 7 September, 1820," Pest, 1820, as well as in the *Berliner Astronomisches Jahrbuch*, for 1821, page 116, and 1822, page 145; subsequently in his "Theoretische und praktische Astronomie," Wien, 1821, part ii., page 280; and last in his "Vorlesungen über Astronomie," Wien, 1830, part i., page 306. Since then numerous applications have been made thereof. My father expressed the well-founded desire that in the astronomical almanacs formulæ might be given similar to that communicated in NATURE.

CHARLES DE LITROW

Vienna, June 1

Acoustical Phenomena

IN connection with Doppler's disputed theory of the colours of stars, the illustration usually employed to assist the mind in forming a conception of the hypothesis is that of the whistle of a passing locomotive. The note of the whistle, which, as it

approaches, seems shriller than its normal pitch, owing to the greater number of vibrations impinging upon the ear in the unit of time, falls half a tone more or less, as the engine passes and recedes. To unmusical ears the difference in the note is a very doubtful fact, only to be taken on hearsay. There is, however, another fact of kindred nature to which attention has not, I believe, been generally drawn. Almost all railway engines, and especially those drawing heavy goods' trains, have, owing to the manner in which the valve-gearing is set, the property of producing the well-known *staccato* puffs of steam, audible to the ear as well as evident to the eye. Anyone who will listen to these puffs as the train dashes by will be aware of a very distinct and well-marked change in their apparent rapidity of succession at the moment of passing. So distinct is the change that almost invariably the first effect on the mind is the illusory suggestion that the train has suddenly slackened speed. This change is heard best at night, and when the passing train is a heavy one, not running too quickly. It cannot fail to be appreciated even by non-musical ears. As an illustration of a scientific principle it is, perhaps of the greater value, as a popular error seems to exist on the subject of the change of the note of the whistle, to the effect that the lowering in pitch is very gradual during the approach and recession of the engine, an opinion obviously incorrect if the observer be close to the train.

London, June 7

S. P. THOMPSON

Giant Tortoises

IN NATURE, vol. xiv. p. 60, it is stated that Commander Cookson, of H.M.S. *Petrel*, is bringing home two live specimens of the giant tortoise of the Galapagos; that *if* their food lasts, and *if* they are not killed by the cold off Cape Horn, they will be the first specimens seen alive in this country.

Even should the tortoises survive the two *ifs* above given, they will not be the first living specimens seen in this country.

A large specimen brought from the Galapagos Islands by one of the ships of the late S. R. Graves, M.P., lived in good health for nearly ten years in our Dublin Zoological Gardens.

This animal was examined, after death, by Dr. Günther, who states that it is not identical with the Indian species, as supposed by former naturalists.

SAMUEL HAUGHTON,
Secretary Royal Zoological Gardens,
Dublin

Trinity College, Dublin, June 2

Photography of the Loan Collection Apparatus

THE Loan Collection of Scientific Apparatus at South Kensington contains many apparatus, as for instance the first air-pump of Otto von Guericke, the first boiler of Papin, the first locomotive, &c., which for the friends of science will ever be of great historical interest. Therefore I cannot refrain from expressing the wish that opportunity should be given to take photographs of convenient size of some of the most interesting apparatus. I believe many visitors will feel with me greatly gratified if such a more enduring remembrance could be taken home of an exhibition that perhaps for ever will remain unequalled.

The Hague, June 12

L. B.

ABSTRACT REPORT TO "NATURE" ON EXPERIMENTATION ON ANIMALS FOR THE ADVANCE OF PRACTICAL MEDICINE

THE courteous request of the editor of NATURE that I should contribute to his pages an abstract of my experience of the value of experimentation on animals and on the most useful applications of that method of research to the alleviation, directly or indirectly, of animal suffering in all the higher classes of animals is responded to in the subjoined notes.

I have already expressed my views on this subject on two occasions at large public meetings of the Royal Society for the Prevention of Cruelty to Animals, and in 1862 I made a report on the same subject to the indefatigable secretary of that society, Mr. Colam, which report he has recently published, and which on the points it refers to is in harmony with the conclusions of the late Royal Commission. I have not, however, entered into the discussion that for some months past has been in

progress, and this for the simple reason that in the violence, I had almost said distemper, of the controversy, I felt I could take no part. In what I am now about to record I shall merely bear witness of what I know without prejudice to either side. I state this at once because I feel morally sure that if I had not been a physician, and if I had not from that circumstance studied the question in connection with human suffering in its most poignant aspects, I should have been one of the strongest partizans amongst those who are most strongly opposed to experimentation. I differ indeed only from them in that I have been obliged to consider the pains of men, women, and children in my daily labours, and have been forced to the conviction that the actual suffering of the inferior animals bears no comparison with that which is borne by the human family; that the mental sufferings alone of man exceed the physical pains of the lower creatures; and that his physical pain is greater in amount, in intensity, and in appreciation.

For my part, the experience I have gained from experimentation has, from the beginning to the end, through a long period of twenty-six years—during which it has at intervals been sought—sprung in almost every instance, directly from the desire to apply scientific research to the instant use of the practising physician. With rare exceptions every inquiry has been prompted by some painful difficulty that has been suggested at the bedside of the sick, or by the sight of operation on the human subject.

If, therefore, experiment on animals can be vindicated by its application to practice, my experience may be of use in settling doubts in the minds, at least, of those who are not unduly biassed on either side.

Experimentation on Death from Chloroform

The first series of experiments I remember to have made were commenced in the years 1850 and 51, and had reference to the mode and cause of death under chloroform. At the time named chloroform had been in use a little over two years, for preventing the pain of surgical operations, and already nineteen deaths in man had occurred from it.

These calamities had produced very painful and anxious feelings amongst medical men, and my researches had for their intention the elucidation of many points of practical importance. The mode of procedure was to narcotise the animals with varying degrees of rapidity, with varying percentages of chloroform vapour in the atmosphere, and during various atmospheric conditions: to note carefully the phenomena produced on the heart and on the respiration, and the duration of the four stages of narcotism. In some instances the animals—rabbits were usually subjected to experiment—were allowed to recover; in other instances the narcotism was continued to death. When the narcotism was made to be fatal the immediate cause of death was noted, and the body was left until the rigidity of death could be recorded. Then all the organs were carefully inspected in order to see what was the condition of the lungs, the heart, the brain, the spinal cord.

The results obtained by these inquiries were of direct practical value. By them I showed in various lectures and papers the following major facts:—

1. That the cause of the fatality from chloroform does not occur, as was at first supposed, from any particular mode of administration of the narcotic.
2. That chloroform will kill, in some instances, when the subject killed by it exhibits, previous to administration, no trace of disease or other sign by which the danger of death can be foretold.
3. That the condition of the air at the time of administration materially influences the action of the narcotic vapour. That the danger of administration is much less when the air is free of water vapour and the temperature is above 60° but below 70° Fahr.
4. That there are four distinct modes of death from

chloroform, and that when the phenomena of death from its application appear, they are infinitely more likely to pass into irrevocable death than from some other narcotics that may be used in lieu of chloroform.

5. That all the members of the group of narcotic vapours of the chlorine series, of which chloroform is the most prominent as a narcotic, are dangerous narcotics, and that chloroform ought to be replaced by some other agent equally practical in use, and less fatal.

6. That so long as it continues to be used there will always be a certain distinct mortality arising from chloroform, and that no human skill in applying it can divest it of its dangers.

That knowledge of this kind respecting an agent which destroys one person out of every two thousand five hundred who inhale it was calculated to be useful no reasonable mind, I think, can doubt. To me who, many hundred times in my life have had the solemn responsibility of administering chloroform to my fellow-men, it was of so much value that I should have felt it a crime if I had gone blindly on using so potent an instrument without obtaining such knowledge.

Experimentation with reference to the Deposition of Fibrine in the Heart, and Prevention of Death from that Cause.

From 1851 to 1854 I was closely occupied in the study of that mode of death which is caused by the separation of the fibrine of the blood in the cavities of the heart. At the time named a medical controversy which had been all but silent for a hundred and fifty years, on the question whether the separations of fibrine which are often found in the heart after death are formed before death and are a cause of death, or are formed after death and are a mere consequence, was revived and was carried on, with much activity, by physicians of different schools. I took a leading part in supporting the view that the separations of fibrine took place, as a rule, before death, and were the cause of death. I did a great deal to prove the truth of this then controverted, and now universally admitted, position, and I gave the first detailed description of the symptoms which indicate the formation of the clots in the cavities of the heart. The result was that I soon became too sadly familiar with this class of case, for I found that the symptoms, whenever they were fairly pronounced, indicated the certain death of the sufferer. These observations led me, naturally, to look for a remedy; to an endeavour to find a means by which the clot of fibrine in the heart could be made to undergo solution. Taking clots that had been removed from the dead and had been causes of death, I subjected them to different solutions to determine their solubility. I found them soluble in some alkaline solutions, and amongst other solutions in ammonia. I also observed that ammonia added to blood held the fibrine of the blood, from which these clots are formed, in solution. The fact led me to expect that by the use of such alkaline solutions a true solvent remedy might be found. A case occurred in which symptoms of fatal character were fully developed, and in the hope of producing solution of the coagulum in the heart, full doses of bicarbonate of ammonia were repeatedly administered. To my great satisfaction the signs of oppression at the heart ceased, life was evidently prolonged, and a fair chance of recovery was presented. The hope of recovery was in a few hours, however, destroyed; coma supervened, and the patient died from that added cause of death. The *post-mortem* revealed that the blood throughout the body was fluid, and that the clot which had been in the heart had undergone all but complete solution. But the red corpuscles of the blood were found also to have undergone the extremest disintegration, and the brain and other vital organs were intensely congested.

The inference I drew at this time, it was in 1854, from

the example in question, was that the remedy which had caused solution of the coagulum had saved life by that process to destroy life by the extension of the solvent action to the blood corpuscles, and this opinion was so fully confirmed by experimentation, that I gave up further inquiry on the subject, from the feeling that its continuance was not warranted. A period of seventeen years now elapsed, in every year of which I had occasion to see from five to six instances of death from this one cause. Some of the deaths from the cause named occurred after surgical operations, such as ovariectomy, some from croup and other inflammatory affections, others before or after childbirth. In 1870 I computed that I had witnessed ninety-seven of these fatal catastrophes. Meantime there had been found no remedy, but I had learned from the added experience one new fact, viz., that in three instances, although no ammonia or other solvent of the blood had been employed in treatment, the symptoms of coma supervened precisely as in the case where ammonia had been administered. At last I obtained one clear evidence that the reason of the symptoms was a separation of fibrine in the sinuses of the brain.

Recurring once more to the use of ammonia as a solvent of the deposited fibrine, I thought it justifiable now to renew experiment. It might, I felt, be the best course to administer the simple liquid ammonia instead of a salt of that substance, by which means I hoped the solvent action would be obtained by an agent that was more easily eliminated from the body when the administration of it was withdrawn.

To what extent I might administer the solvent, how far I might venture to produce disintegration of the corpuscles of the blood and hope for recovery, was the point to be arrived at. It could only be arrived at by one of two methods—by trying the experiment on the inferior animals, or by waiting for the opportunity of testing the remedy directly on man in some extreme case of the diseased condition specified. I chose, and I think correctly, the first of these alternatives. I subjected an animal, a guinea pig, to the administration of ammonia diluted as it might be for the human subject, and I continued the administration until I found, firstly, that life was possible and safe under a degree of solution of blood which in the absence of such a direct test would have been thought impossible; and secondly, that on the withdrawal of the solvent agent the natural state was slowly but completely restored. I repeated the research in order to test the best mode of administration. I tried on myself the doses that could be swallowed without actual pain, and then I planned the measure I would adopt when another instance of obstruction of the blood in the heart came under my care. I need not repeat here, in any detail, the satisfactory results of this inquiry. The facts have been recorded at length before the Medical Society of London, have been made widely known in the profession of medicine, and have gathered confirmation from others. It is sufficient for me to state that in 1872, in an example of this fibrinous obstruction in the heart, when the sufferer was to all known observation *in extremis*, the treatment by ammonia, in doses which would have been considered poisonous had not experiment on animals proved the contrary, was pushed to the full; that the evidence of solution of the obstructing mass in the heart was perfect; and that complete recovery, I have no doubt the first recovery of the kind, was the result. Since then I know of eight more examples in which the same rational method of treatment has been applied, with the result of six recoveries.

Experimentation for Surgical Learning.—Ovariectomy.

I have sometimes had occasion to perform, or take part in experiments on the lower animals in order to learn some important detail of *surgical* practice. The following experience of this nature is worthy of special note.

When Mr. Spencer Wells was beginning his career in performing the operation of this century,—the removal of ovarian tumours,—a difficulty arose on the point whether in closing up the wound in the abdomen the peritoneum ought or ought not to be included in the stitches. At the present time, when so much is known, this subject may appear of little moment; then it was of vital moment. The peritoneum had been held by all authorities to be of such importance in the animal economy that to cut or injure it was thought to be actually a deadly act, and a man who intentionally injured the peritoneum, in operation, was considered, by many, as little better than a wanton and wicked experimenter on human life. Ought any one, therefore, to venture to put two rows of stitches through this structure? Mr. Wells wished to ask the question of nature, by experiment, and I helped him. Eighteen animals of three classes—guinea-pigs, rabbits, and dogs—were first thoroughly narcotised. Then the same incision was made into the abdominal cavity as is made in ovariectomy. Afterwards the incision was neatly and closely sewn up, in one set of experiments with the peritoneum included in the stitches, in the other set with the peritoneum excluded. The animals, on coming out of their sleep, were attended to and treated with as much care as if they had been human until their recovery, which in each case was rapid and easy. When they had entirely recovered and the wound healed, they were submitted to painless death, under anaesthesia, and their bodies were examined to determine the results of the different modes of operation.

These were the steps of the proceeding. The lessons taught were of vital value. The experimentation proved beyond dispute that the introduction of the stitches through the peritoneum added no danger to the operation. They proved further that when the peritoneum was included in the stitches, the wound healed much more firmly and safely, *a fact which could only have been learned from an operation on a subject that could be killed after operation*. From that time of probationary learning on to this time of matured experience, Mr. Wells has performed the great operation, with which his name is for ever identified, 770 times. In every instance the patients who have come under his care for operation would, presumably from past experience, have died from the disease. Of his patients operated on an average of three out of four have recovered. He has, therefore, by his own hand saved between five or six hundred women from one form of certain and lingering death. Towards this result—a result grander than has ever before fallen to the lot of any operator of any age—he was fortified by the experiments I have described to an extent which no one but an operator himself can fully appreciate.

I am aware there are some who would urge that he might have learned the facts he wanted to obtain by experience, that is to say he might have waited for the results from his operations on women. This plan would have made several women in the prime of life subjects of experimental inquiry. I am aware that some would say it were better the operation had been dropped than that any animal whatever had been subjected to suffering for its sake. This plan would have been an obstacle to the saving of over five hundred women from early and certain death in the practice of Mr. Wells alone. But when it is remembered that his teaching and example have been followed wherever surgery is practised, the numbers of women saved from death and suffering during the last fifteen years in consequence of what was learnt by sacrificing some eighteen dogs, rabbits, and guinea-pigs, it is obvious that those who estimate human life at its real value and observe human suffering in its most distressing forms are compelled, however painful to their own feelings, to think and act first for the best interests of the human family.

What Lord Selborne, one of our most distinguished

Chancellors, thinks of the results of Mr. Wells's work may be gathered from one of his published speeches. He calls it "one of the most splendid triumphs of modern surgical art and modern philanthropy, one of the greatest achievements of medicine or surgery in any age." Mr. Wells himself has repeatedly urged that what he learnt by the result of the experiments we performed together has been of the utmost importance for the success of the operation, and in a note addressed to me to-day he repeats and permits me to publish his views in his own words:—

"The few experiments we made on the narcotised animals taught in a few weeks, in the early days of ovariotomy, what I could not have learned to this hour, after many years' observations on suffering women. To my mind, the loss to the world by the few animals sacrificed, when compared with the gain by the lives of the thousands of suffering women already saved, wherever the improved methods of operating learned by these experiments has been followed, is so utterly disproportionate as not even to be worthy of consideration."

BENJAMIN W. RICHARDSON

(To be continued)

OUR ASTRONOMICAL COLUMN

THE COMET OF 1698.—The orbit of the comet of 1698, which appears in our catalogues was calculated by Halley from the observations of Lahire and Cassini at Paris. In his "Synopsis of the Astronomy of Comets" he remarks that the comet "was seen only by the Parisian observers who determined its course in a very uncommon manner. This comet was a very obscure one, and although it moved swiftly, and came near enough to our earth, yet we, who are not wont to be incurious in these matters, saw nothing of it."

The comet was detected on Sept. 2, between β and κ Cassiopee, and thence pursued a southerly course until on the 28th of the same month it was last observed between ξ and ψ Scorpii. On calculating geocentric places from Halley's orbit, it appears that the elements as originally published by him, and as they have been successively copied into all our catalogues, give an apparent track in the heavens which is totally different from that recorded by Lahire and Cassini, and described in *Anciens Mémoires de l'Académie des Sciences*, t. x., and which is traced on the chart in the *Mémoires* for 1702. Employing positions deduced as closely as practicable from the somewhat imperfect details in our possession, for Sept. 2, 15, and 28, the following orbit results:—

Perihelion Passage 1698, Sept. 17 ⁰² 14	Paris mean time.
Longitude of the perihelion ...	274° 42' } Equinox
" Ascending node ...	65 53 } of 1699.
Inclination ...	10 55
Log. Perihelion Distance ...	9.86252
Heliocentric motion — retrograde.	

On comparing these elements (which very fairly represent the apparent track of the comet) with Halley's, it is at once evident that the cause of the failure of the latter is the substitution in the "Synopsis" of the longitude of the *descending*, instead of that of the *ascending* node, an oversight which appears to have escaped detection hitherto. Making this change in Halley's elements they will stand as follows:—

Perihelion Passage 1698 Oct. 18 at 16h. 57m.	Greenwich
Longitude of Perihelion ...	270° 51' 15" time.
" Ascending Node ...	87 44 15
Inclination ...	11 46 0
Log. perihelion distance ...	9.83966
Motion — retrograde.	

The first orbit appears to agree better upon the whole with the path of the comet laid down in the above mentioned diagram.

THE BINARY STAR ω LEONIS.—In the "Transactions of the Royal Irish Academy," vol. 26, Dr. Doberck has given the details of a very elaborate determination of the orbit of this star, on measures extending to the spring of the present year. Mädler had previously given two orbits, Villarceau one, and Klinkerfues three, so that the object had not been neglected, but a longer course of measures than had been employed by these calculators was required for a trustworthy approximation to the orbit. Dr. Doberck presents the following elements as definitive for the present:—

Peri-astron passage ...	1841.81
Node ...	148° 46'
Angle between the lines of nodes and apses (λ) ...	121° 4'
Inclination ...	64° 5'
Excentricity ...	0.5360
Semi-axis Major ...	0".890
Period of revolution ...	110.82 years.

From these elements we deduce the following angles and distances, exhibiting the course of the companion during the present century:—

1878.0	Pos. 75° 1'	Dist. 0".56	1890.0	Pos. 102° 4'	Dist. 0".75
82.0	" 86° 0'	" 0".62	94.0	" 108° 5'	" 0".82
86.0	" 95° 0'	" 0".68	98.0	" 113° 6'	" 0".89

Some remarks on the correction of orbits of double stars, appended by Dr. Doberck to his paper on ω Leonis, one of the most complete of the series emanating from Col. Cooper's Observatory at Markree Castle, may be useful to those who are occupied with these orbits.

VARIABLE STARS.—(1) Olbers' supposed variable, near 53 Virginis. Mr. J. E. Gore, writing from Umballa, Punjab, on May 13, says he examined the place of this star a few nights previous with a 3-inch refractor, and found it about 9 m., being about equal in brightness to Olbers' star c , and brighter than his star d , which latter appeared more nearly 9½ or 10 than 11, as given by Olbers. With an opera-glass the suspected variable was "about the faintest star in the immediate vicinity of 53 Virginis."

(2) 5 Ceti.—Recent observations afford a suspicion of variability to a small extent in this decidedly reddish star, which, by the way, is not found in Schjellerup's second catalogue of objects of this class. It may be advantageously compared with its neighbours 4 Ceti and B.A.C. 5. (3) The Companion of Algol. The small star near β Persei, appears to have been first remarked by Schröter, on October 12, 1787, with a 7-foot telescope, power 160; on November 3 the distance was estimated 1' 30". On April 9, 1788, he could not find the small star, and hence concluded it to be variable. Observations during the last two or three years have rather indicated fluctuation of brightness, the star being sometimes caught at once, and at others only perceived with difficulty, employing the same telescopes and on nights not differing materially in transparency. It would not be without interest to ascertain definitely by systematic observation whether there is any ground for the suspicion first entertained by Schröter.

THE DOUBLE-STAR γ CENTAURI.—Will one of our southern readers put upon record the actual angle of position and distance of this object, to decide upon the direction and amount of the motion, which at present are by no means obvious? Capt. Jacobs' measures in December, 1857, showed that the star was widening, as compared with his estimate in March of the preceding year, but he found a retrograde change of angle to the amount of 7°, whereas the angle of 1856, compared with Sir John Herschel's measures in 1835–36, rather point to direct motion. Capt. Jacobs says, in 1857, "Has opened sensibly since 1853, being now an easy object, whereas then, under the most favourable circumstances, it could only just be discerned as not round."